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GAMBLING SUPERSTITIONS.

By Richard A. Proctor.

(*Continued from page* 80.)

BUT it may not seem quite so easy to explain those undoubted runs of luck, by which players "in the vein" (as supposed) have broken gaming-banks, and have enabled those who have followed their fortunes to achieve temporary success. The history of the notorious Garcia, and of others who like him have been for awhile the favourites of fortune, will occur at once to many of my readers, and will appear to afford convincing proof of the theory that the luck of such gamesters has had a real influence on the fortunes of the game. The following narrative gives an accurate and graphic picture of the way in which these "bank-breakers" are followed and believed in, while their success seems to last.

The scene is laid in one of the most celebrated German Kursaals.

"What a sudden influx of people into the room! Now, indeed, we shall see a celebrity. The tall, light-haired young man coming towards us, and attended by such a retinue, is a young Saxon nobleman who made his appearance here a short time ago, and commenced his gambling career by staking very small sums; but, by the most extraordinary luck, he was able to increase his capital to such an extent that he now rarely stakes under the maximum, and almost always wins. They say that when the croupiers see him place his money on the table, they immediately prepare to pay him, without waiting to see which colour has actually won, and that they have offered him a handsome sum down to desist from playing while he remains here. Crowds of people stand outside the Kursaal doors every morning, awaiting his arrival, and when he comes following him into the room, and staking as he stakes. When he ceases playing they accompany him to the door, and shower on him congratulation and thanks for the good fortune he has brought them. See how all the people make way for him at the table, and how deferential are the subdued greetings of his acquaintances! He does not bring much money with him, his luck is too great to require it. He takes some notes out of a case, and places maximums on *black* and *couleur*. A crowd of eager hands are immediately outstretched from all parts of the table, heaping up silver and gold and notes on the spaces on which he has staked his money, till there scarcely seems room for another coin, while the other spaces on the table only contain a few florins staked by sceptics who refuse to believe in the Count's luck." He wins; and the narrative proceeds to describe his continued successes, until he rises from the table a winner of about one hundred thousand francs at that sitting.

The success of Garcia was so remarkable at times as to affect the value of the shares in the *Privilegerte Bank* ten or twenty per cent. Nor would it be difficult to cite many instances which seem to supply incontrovertible evidence that there is something more than common chance in the temporary successes of these (so-called) fortunate men.

Indeed, to assert merely that in the nature of things there can be no such thing as luck that can be depended on even for a short time, would probably be quite useless. There is only one way of meeting the infatuation of those who trust in the fates of lucky gamesters. We can show that, granted a sufficient number of trials—and it will be remembered that the number of those who have risked their fortunes at *roulette* and *rouge et noir* is incalculably great—there must *inevitably* be a certain number who appear exceptionally lucky; or rather, that the odds are overwhelmingly against the continuance of play on the scale which prevails at the foreign gambling tables, without the occurrence of several instances of persistent runs of luck.

To remove from the question the perplexities resulting from the nature of the above-named games, let us suppose that the tossing of a coin is to determine the success or failure of the player, and that he will win if he throws "head." Now if a player tossed "head" twenty times running on any occasion it would be regarded as a most remarkable run of luck, and it would not be easy to persuade those who witnessed the occurrence that the thrower was not in some special and definite manner the favourite of Fortune. We may take such exceptional success as corresponding to the good fortune of a "bank-breaker." Yet it is easily shown that with a number of trials which must fall enormously short of the number of cases in which fortune is risked at foreign Kursaals, the throwing of twenty successive "heads" would be practically *ensured*. Suppose every adult person in Britain—say 10,000,000 persons in all—were to toss a coin, each tossing until "tail" was thrown; then it is practically certain that several among them would toss twenty times before "tail" was thrown. Thus: It is certain that about five millions would toss "head" once; of these about one-half, or some two millions and a-half would toss "head" on the second trial; about a million and a quarter would toss "head" on the third trial; about six hundred thousand on the fourth; some three hundred thousand on the fifth; and by proceeding in this way — roughly halving the numbers successively obtained — we find that some eight or nine of the ten million persons would be almost certain to toss "head" twenty times running. It must be remembered that so long as the numbers continue large the probability that *about* half will toss "head" at the next trial amounts almost to certainty. For example, about 140 toss "head" sixteen times running: now, it is utterly unlikely that of these 140, fewer than sixty will toss "head" yet a seventeenth time. But if the above process failed on trial to give even one person who tossed "heads" twenty times running—an utterly improbable event—yet the trial could be made four or five times, with practical certainty that not one or two,

but thirty or forty, persons would achieve the seemingly incredible feat of tossing "head" twenty times running. Nor would all these thirty or forty persons fail to throw even three or four more "heads."

Now, if we consider the immense number of trials made at gambling-tables, and if we further consider the gamblers as in a sense typified by our ten millions of coin-tossers, we shall see that it is not merely probable, but absolutely certain that from time to time there must be marvellous runs of luck at *roulette, rouge et noir, hazard, faro,* and other games of chance. Suppose that at the public gaming-tables on the Continent there sit down each night but one thousand persons in all, that each person makes but ten ventures each night, and that there are but one hundred gambling nights in the year—each supposition falling far below the truth—there are then one million ventures each year. It cannot be regarded as wonderful, then, that among the fifty millions of ventures made (on this supposition) during the last half century, there should be noted some runs of luck which on any single trial would seem incredible. On the contrary, this is so far from being wonderful that it would be far more wonderful if no such runs of luck had occurred. It is probable that if the actual number of ventures, and the circumstances of each, could be ascertained, and if any mathematician could deal with the tremendous array of figures in such sort as to deduce the exact mathematical chance of the occurrence of bank-breaking runs of luck, it would be found that the antecedent odds were many millions to one in favour of the occurrence of a certain number of such events. In the simpler case of our coin-tossers the chance of twenty successive "heads" being tossed can be quite readily calculated. I have made the calculation, and I find that if the ten million persons had each two trials the odds would be more than 10,000 to 1 in favour of the occurrence of twenty successive "heads" once at least; and only a million and a half need have a single trial each, in order to give an even chance of such an occurrence.

(*To be continued.*)

CAUSES OF THE GLACIAL PERIOD.

(*Continued from page* 39.)

HAVING considered in our last article the more probable astronomical causes, we will now proceed to discuss the remaining geographical and other causes which have been alleged as contributing to bring about the great Glacial Period, dealing, in the first instance, with the probable geographical causes.

1. *Distribution of Land.*—The effect of land upon climate is in tropical countries to increase the heat and in arctic ones to increase the cold. During summer also the effect of land is to increase the temperature, and during winter to decrease it. Hence it is, that the land in Arctic regions usually possesses a much more severe climate than the adjoining seas, frozen though these may be. Owing to such being the case, Sir Charles Lyell, the eminent geologist, was led to the belief that the Glacial Period was principally due to an abnormal condition in the distribution of the land at that time. He was of opinion that an excess of land in polar regions (combined with an absence of land in equatorial regions) was the chief cause which led to the Glacial Period. For instance, he considered that both Europe and America might at that time have been united to Arctic regions by large tracts of land now submerged, and that a great extent of land then existed round the North Pole. Now, although such a state of things would, in all probability, have contributed considerably to a glacial climate, there seems to be scarcely evidence sufficient to warrant an assumption that the distribution of land was so very different during the Glacial Period to what it is now. With the exception of the union of England to the Continent during part of that period, and the somewhat further extension of the European continent westwards, proofs are wanting of any very important difference between the geography of that time and this, and the differences of which we have evidence do not justify us in setting them down as the chief cause of the Glacial Period.

2. *Elevation of Land.*—Altitude or elevation of land has also a great effect upon climate, its effect being always to decrease the temperature. Some have supposed, therefore, that during the severity of the Glacial Period the land in the Northern Hemisphere was more elevated above the sea level than at present. But, although there is abundant evidence in England of a submergence during that period—witness the occurrence of Arctic shells on some of the Welsh mountains—there is not sufficient evidence of a general elevation very much exceeding the present state of things. Consequently, this supposition suffers in the same way as the last one discussed; and, although both may have been favourable to the occurrence of a Glacial Period, neither can be said to have been demonstrated as the primary cause of the same. Moreover, we have seen that mild intervals in all probability occurred during the Glacial Period, and the jumping up and down of land on purpose to explain these alternate mild and severe intervals of climate, is a somewhat violent supposition.

3. *Winds.* The southern part of Europe is at present warmed considerably by the south winds blowing from the hot desert of the Sahara. But the Sahara bears signs of having been only (geologically speaking) a recent upheaval of the bed of the sea. Consequently, formerly the south winds would not be charged with the burning heat they have now, and would not possess the power they now have of melting the snow on the mountains, and stopping the advance of the glaciers of Switzerland and Italy. The absence of these warm winds could not fail to contribute to the severity of the climate of Europe.

4. *Currents.*—Of more importance to our climate than winds are ocean currents, especially that most considerable of all ocean currents, the Gulf Stream. Our readers will probably be aware that there is a great difference between the climate of Europe and North America in the same latitudes. For instance, the harbour of St. John, Newfoundland, which is often blocked by ice, not only in winter, but as late as May and June, is actually further south than the principal seaports of England and France which enjoy open harbours all the year round. Boston, in America, possesses only the same mean annual temperature as Dublin, although it is 11° further south; while that part of North America in the same latitude as the British Isles comprises the desolate and ever-frozen regions of Labrador and the Hudson's Bay territory. This great difference between the climate of the east and west shores of the Atlantic is due in a great measure to the warm waters of the Gulf Stream poured upon the shores of England and Ireland, so that it is easy to see what a vast difference would be made in our climate if by any means the Gulf Stream were diverted. Now, the great central valley of the Mississippi is nowhere more than 800 ft. above the sea level, and it has been urged that if this valley were lowered a direct course would be given to the Gulf Stream up the centre of North America; also, that if the Isthmus of Panama were submerged, its course would also be

diverted, and, in either case, that England and Western Europe would thereby be reduced to a condition of climate similar to that of Hudson's Bay, Labrador, and Newfoundland at the present day—in fact, so great would be the results of an alteration in the course of the Gulf Stream upon the climate of Europe, that some eminent writers have considered that that alone would amply account for the Glacial Period. But we must not forget that evidences of such a period exist elsewhere than in Europe, and that even in America marks of glacial action are found 10° further south than in Europe, which fact points to relative conditions of climate in the two continents similar to those now subsisting, and also tends to show that the Glacial Period *in both* was due to general rather than local causes. Moreover, as I have already hinted, a glacial condition of the northern hemisphere caused by the northern winter occurring in aphelion during a period of great eccentricity of the earth's orbit would probably induce various changes in the winds and currents of the globe; so that instead of such changes causing the Glacial Period, as some have contended, the Glacial Period might, on the contrary, have caused them. However this may be, it is certain that such changes, however caused—especially changes in the direction of the Gulf Stream—would, in a very considerable degree, augment the rigour of the glacial climate.

We have now considered the causes, both astronomical and geographical, which most probably led to the Glacial Period; but, as I have before said, it was in all probability a combination of those causes, rather than any one of them, which ultimately brought about such a period in the northern hemisphere. It only remains for us, therefore, in order to render more complete our study of the causes of the Glacial Period to notice briefly two or three other causes which have with less probability been assigned by some.

1. *Changes in the Inclination of the Earth's Axis*, or what amounts to the same thing, in the "*Obliquity of the Ecliptic*," the latter being, of course, dependent upon the former. The axis of the earth at present is inclined to the plane of the orbit or the ecliptic at an angle of 23° 28′; but owing to the attraction of the other planets this inclination of the axis or obliquity of the ecliptic is perpetually varying within certain limits. The effect of this upon climate would be that whenever the angle of obliquity was less than now, the Arctic regions would be more exposed to the long and cold winters in aphelion, and whenever the angle of obliquity was greater the opposite would be the case; so that combined with great eccentricity of the earth's orbit and the other causes I have mentioned, the result of such obliquity would be to increase or mitigate (according to the amount of such obliquity) the severity of climate. As, however, the limits within which variation in the inclination of the earth's axis or obliquity of the ecliptic can take place are very narrow, its effects on climate are probably next to imperceptible.

2. *Supposed Diminution of the Sun's Heat* and *Supposed Passage of the Earth through Cold Regions of Space.* Both these alleged causes are purely conjectural. They involve, moreover, a transparent fallacy. For in either case the temperature of the earth would be lowered generally, and in such a state of things it is difficult to see how ice and glaciers could be formed on land. For we must remember that it is the heat of the sun which draws up the aqueous vapour from the ocean, which vapour afterwards falls in cold climates as snow, and is frozen into glaciers and ice; so that if the sun's heat were in any way diminished, aqueous vapour would not be drawn up as now, and so the very material of which glaciers are formed would be cut off at its source. Thus, although a cold condition of the earth generally might freeze the sea to the bottom, it could never cause a glacial state of things *on land*. Hence we learn the great truth that, paradoxical though it may appear, glaciers and land ice owe their formation ultimately to HEAT, for if there were no heat, or not sufficient heat, the aqueous vapour could not be raised as now from the ocean, and without aqueous vapour there could be no snow, no glaciers, and no land ice. Consequently, we may dismiss these last two alleged causes as altogether worthless, and fall back upon all or some of the causes previously referred to, it being perfectly plain that a diminution of the sun's power, or the passage of the solar system through a space of low temperature, would destroy the glaciers at their source. We therefore see by ultimate analysis of the explanations we have examined that the conditions necessary to produce a Glacial Period are *a short, hot summer and a long, cold winter, with plenty of aqueous vapour caused by the heat, to be converted by the cold into snow and ice.*

ROBERT B. COOK.

PLEASANT HOURS WITH THE MICROSCOPE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

IT is a good plan to keep some wide-mouthed bottles, holding two or three ounces, in a warm, light place to develop the various germs that are sure to be found in the water of a duck-pond, or any other promising source. Larger vessels are likely to yield the greatest variety with hay or other vegetable infusions, but the smaller ones are very handy, and may have the chance of their fertility increased by a few drops of soup or little bits of lean meat, which last favour the development of amœbæ. Often, after several weeks or months, quite unexpected and curious arrivals are found in the bottles, and if a drop looks promising when viewed under the microscope, arrangements should be made to give a whole evening to the study. Now, whether looking through a microscope for hour after hour is fatiguing and eyesight-damaging or not depends upon some points easily attended to.

The usual position of a microscope with a tube slanting a little and the head leaning forward to look down it, is all very well for a short examination of any object, but not at all desirable for continuous work. A better plan is to get a carpenter to make a light stool 2 ft. long and 14 in. wide, standing on four legs, the length of which should be determined by that of the microscope it is intended to use and the height at which the observer sits. My stool is 7 in. high, and when placed on an ordinary table brings a full-sized microscope with its tube in a horizontal position, at a convenient height for the eye of an observer sitting in an ordinary chair. The late Mr. Lobb, who was skilful in exhibiting troublesome objects, always used his microscope in this position, but, so far as I know, it is seldom adopted. When the instrument is in position as described, the substage mirror should be turned out of the way, and the lamp placed so that its flame is exactly opposite the axis of the instrument, and can be seen in the middle of the field on looking through it. If the objects to be watched are large enough for a low power, the light may be softened by placing under the slide a piece of foreign post-paper saturated with spermaceti. For high powers an achromatic condenser is desirable, and one of the smallest central stops is usually the most useful for displaying fine cilia, or delicate whips, as well as for

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lighting up without glare the interior of various creatures. If all is arranged properly, the manners and customs of infusoria may be watched for hours without more fatigue than reading a well-printed book. A tenth part of the time spent with the head leaning forward in the usual way is far more exhausting.

On a recent occasion a slight examination of some water showed that it contained a number of small creatures moving with whips, and that a high power was required to show them well. That employed was a fine oil immersion $\frac{1}{12}$th of Leitz,—an excellent and easily-managed glass. Amongst the most conspicuous objects were round and roundish creatures, relations of the beautiful emerald-coloured, ruby-eyed, and fish-shaped *Euglenæ*. They were *Trachelomonads*, some in smooth shells, others in shells with short spines, something like those on the outer case of chestnuts, but blunter. The specific names *cylindrica* and *hispida* correspond with these peculiarities. The contents of the shells were of an exquisite emerald-green colour, with ruby eye-spots—not really eyes. Each shell was like a bottle with a short neck, out of which came a long, thin whip. The optical edges of the shells looked a fine red colour. Of course, they were not real edges at all, but only perspective terminations of the view, and the colour was produced by the thickness of the material through which the light passed. These creatures can at some period of their existence get out of their bottles, squeezing through the neck, but that did not occur in the case before us.

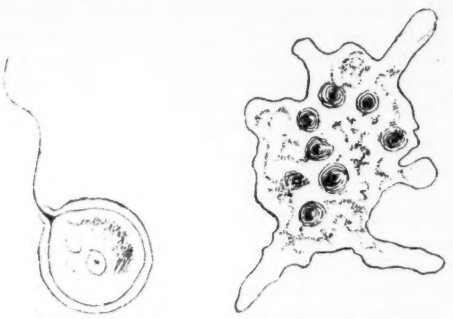
The interest lay in noting the use of the wonderful whip. Some of them used it to keep up a jerking motion without any progression. The whips were then either lashed close round the shell, or thrown forward and then retracted. In most instances, the motion was what would be expected from giving the water a push in one direction, and the little bottle-imp moving in an opposite one. There were, however, some exceptions, and one creature jerked himself a little forward by throwing his whip straight out, and being, as it appeared, more affected by its momentum than by the reaction of the water. The most curious thing occurred with one of the smooth sort, which twirled rapidly round, like a whipping-top, for a quarter of an hour with a velocity that converted its conspicuous dots of colour into circular lines, just as a stick alight at one end makes, when quickly whirled, a ring of fire. As the whips of these creatures look like very fine hairs when magnified a thousand times, this performance with such an instrument is truly wonderful.

What are these things? They were formerly thought to belong to the plants, but Mr. Saville Kent, in his splendid work,* agrees with those who regard them as animals. His description runs as follows:—

Trachelomonads: Ehrenberg. — Animalcules, monoflagellate, plastic, and changeable in form, enclosed within a free-floating ovate or spheroidal indurated sheath, or lorica; the anterior extremity of the lorica perforated by a minute aperture, through which, in its normal condition, the single flagellum only is protruded; oral aperture terminal, followed by a distinct pharyngeal passage; endoplasm coloured green, with usually a red pigment spot at the anterior extremity; contractile vesicle single, spherical, located near the anterior pigment spot. Mostly inhabiting fresh water.

After the slide, with its water-drop containing the Trachelomonads, had been an hour or two under the microscope, a minute rapidly-form-changing *amœba* made its appearance. Most of these animated lumplets of jelly are sedate in their movements. This was not for two seconds of the same shape, but actively putting out pseudopodia of hair-like tenuity, first from one part, and then from another, the main substance flowing after them. Presently there came out of a mass of slime and *débris* a much larger and less excitable specimen, acting with a deliberation that contrasted curiously with its more lively and versatile companion. The *amœbæ* always throw out their pseudopods as clear protoplasm, and then the granular portions, with their multifarious contents of captured prey, flow, or slobber, after them. Watching their doings shows that they act with *intention*, though no one would add the epithet of "conscious."

The bottle of water contained several individuals of another remarkable infusoria, which will be described in the next paper. Subjoined are figures of a *trachelomonad*, and of the larger of the two *amœbæ*.



Trachelomonad. Amœba. ×600.

* "Manual of the Infusoria."

HOW TO MAKE USEFUL STAR MAPS.

By Richard A. Proctor.

(*Continued from page 37.*)

In my last paper I mentioned that when the meridians and parallels of a map had been drawn in according to the simple plan I described, the stars could be filled in from the star-atlas or from any convenient catalogue. I propose now to dwell a little on the peculiarities which render the map constructed on the conical projection a better representation of the particular part of the heavens it exhibits than the corresponding part of a star-atlas can commonly be.

Of course, in the first place, there is the advantage of scale. We can make a representation of a small part of the heavens cover a sheet as large as that used in the star-atlas for a large portion of the celestial sphere. And if star-atlases were constructed on well-conceived plans this would be the only advantage we could get from making maps of separate constellations. Even then such maps would be very useful, because there would be plenty of room in them to pencil down any notes which the amateur might wish to record about a star, or again to track down the course of a comet from the places published in the daily journals, and independently of that, the construction of such maps is a most useful exercise for the beginner, and will, in the long-run, amply repay him for any time he may give to the work.

But there are other and much more important reasons why the construction of such maps as I have described is not only useful but necessary.

That the celestial sphere can be divided into a convenient number of maps, similar in size and shape, and without

any distortion or change of scale worth noticing, I have shown in my "Library Atlas," and School Star-Atlas." No series of maps before published in the form of an atlas satisfied those simple conditions.

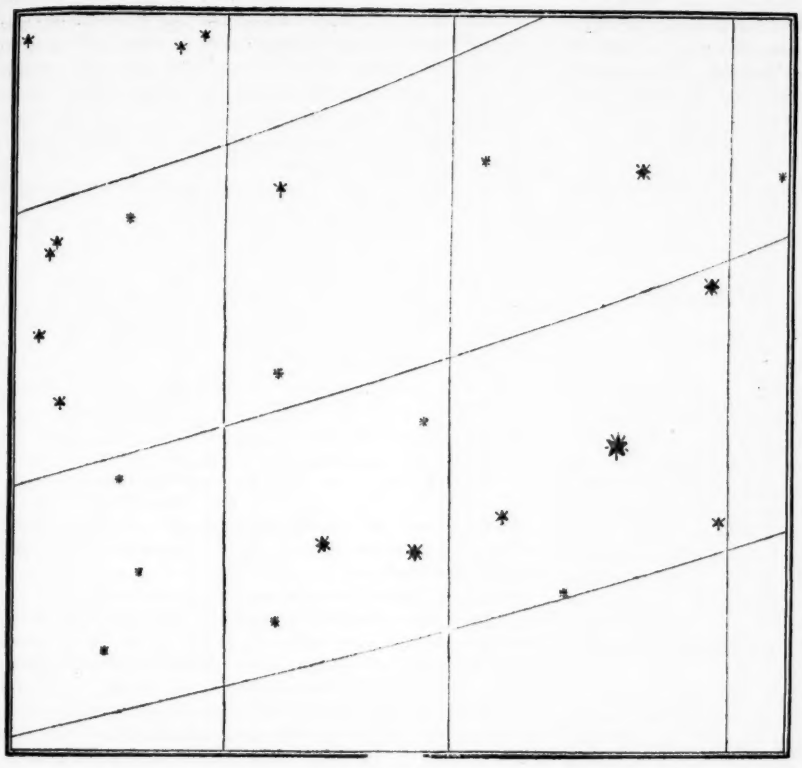


Fig. 5.

Consider, for instance, the large maps published by the Society for the Diffusion of Useful Knowledge, in 1850. These were planned with great skill, to fulfil a condition which many hold to be of the utmost importance for star maps. It was proposed that the heavens should be so exhibited that any three stars which appear to lie in the same straight line on the heavens (or really on what is called a great circle of the sphere), might also lie in a straight line on the maps. The projection which gives this relation is called the gnomonic projection. The heavens were divided into six star-maps, and a simple notion of the plan made use of will be obtained by conceiving the celestial sphere enclosed within a cubical box, and each star marked on a face of this box by drawing a line from the centre of the sphere through the star until it met the corresponding plane face. The maps were made on the scale of a globe two feet in diameter, and they were constructed with an amount of care and a fulness of detail which rendered them, in spite of the inherent defects of the mode of projection adopted, a most valuable series for any one who could interpret their more distorted portions.

Now the stars in Fig. 5 have been pricked off from the S. D. U. K. map. The part of the heavens represented is the well-marked group of stars forming the Northern Crown, which probably formed in very ancient planispheres the uplifted right arm of Boötes—the Herdsman. This part of the heavens does not fall on the most distorted part of the S. D. U. K. map, yet it will be seen at once how largely the shape and position of the spaces inclosed between the meridians and parallels has been distorted. Even in the small space covered by the map, the scale changes perceptibly, as is shown by the rapid increase in the width of the vertical spaces, and by the acuteness of the angles which ought to be right angles.

But the full extent of the distortion can only be recognised by comparing the actual aspect of the star-group, exhibited in Fig. 6, with its distorted presentation in the S. D. U. K. map. In Fig. 6 the scale is precisely the same as in Fig. 5, each map being on the scale of a 2 ft. globe. But Fig. 6 being laid down on the conical projection—that is, according to the simple plan which I have described—there is no appreciable distortion in it; we see all the spaces between the same parallels similar in shape. All the angles are appreciably right angles, and everything, in fact, is clear and intelligible.

Now, on comparing Fig. 6 with Fig. 5, we see immediately that the whole group of stars in Fig. 5 has been distorted. The stars form a sort of spiral in both figures, commencing at the lower left-hand corner, going thence to the upper left-hand corner, next to the upper right-hand corner, and so (by continuing the sweep) into the middle of the map. But whereas in reality the bend at the upper left-hand corner is somewhat acute, the S. D. U. K. map represents it as obtuse; while the almost circular sweep repre-

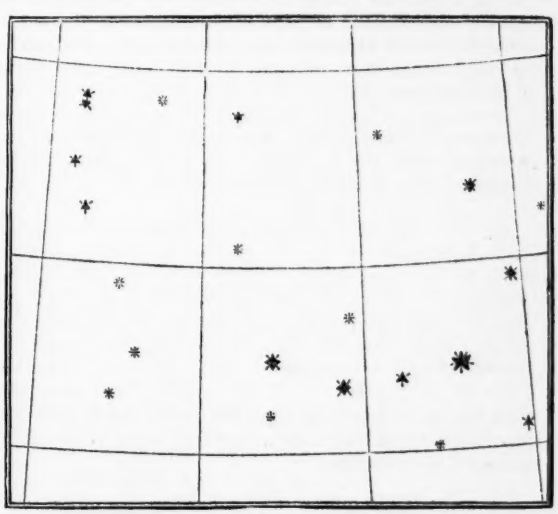


Fig. 6.

sented on the right of Fig. 6 (omitting four small stars), is changed in Fig. 5 to quite another figure.

I do not wish to be understood as in any way finding fault with the S. D. U. K. maps; unfortunately no series of maps can have every good quality we can desire. If we wish to preserve the straight-line pointing, which is the essential property of the S. D. U. K. maps, we must sacrifice some other properties. That I believe that property to be very valuable for popular maps, I have shown by the construction of my own series of gnomonic maps, in which the heavens are divided into twelve instead of six equal portions, and so more than three-fourths of the distortion saved. But in all gnomonic maps, except very small ones, there must be appreciable distortion; and we see at once, therefore, the necessity of supplementing them, so to speak, by the construction of undistorted maps of all the principal star groups.

(To be continued.)

A YEAR'S WEATHER FORECASTS.

By John W. Stainforth.

PART I.—THE WIND.

IT has been said that the amount of work done in connection with the science of astronomy varies inversely as the amount of instrumental means possessed by the worker. If this be true of astronomy it is equally true of meteorology, and I think it will be granted that in testing the "Weather Forecasts of the Meteorological Office," and indicating their weak points, there is a field of useful work before those who are not even possessed of what are usually regarded as indispensable adjuncts of all meteorological observation—a barometer and a thermometer. Such is the course I have adopted during the past year, and it is the object of this paper to place before the readers of Knowledge the results obtained.

Bearing in mind that the Meteorological Office confines its attention, as a rule, to the wind's direction, the wind's force, and the general state of the weather (rainy, fair, foggy, &c.), it will be obvious that in order to ascertain the value of the forecast it was necessary to obtain a trustworthy record of these several atmospheric conditions. I shall, therefore, deal with them under their separate heads.

I. *The Wind's Direction.*—This was ascertained by means of observations of nine vanes which were passed at different periods of every day, and by noting the run of the lower clouds when no vanes were in sight. The result so obtained was checked by means of the very complete Meteorological Reports published by our two Sheffield papers, and the final record entered in my Weather Book. In order to test the forecast, I adopted a plan somewhat similar to that employed by Mr. Jude (Vol. iii., p. 168), and classed the results under four heads.

When the direction of the wind was within the prophesied limits I denoted the fact by A^4; when it exceeded those limits by less than 45°, by A^3; by more than 45°, but less than 90°, by A^2; and when the direction deviated more than 90° from the prophesied limits, by A^1. These letters, together with those referred to afterwards, were entered by the side of each day's record in my book. Like Mr. Jude, I should be inclined to call A^4 correct, A^3 nearer right than wrong, A^2 nearer wrong than right, and A^1 incorrect. In the following table the results of such a system are given for the last year:—

	A^4	A^3	A^2	A^1
Jan.	20	3	1	2
Feb.	18	4	1	0
March	15	7	3	2
Carried forward ...	53	14	5	4
April	12	3	5	5
May	19	3	0	4
June	14	5	2	4
July	22	2	1	1
Aug.	19	4	2	2
Sept.	10	6	3	5
Oct.	20	3	2	1
Nov.	13	6	3	1
Dec.	17	5	0	0
Totals	199	51	23	27

In other words, out of the three hundred forecasts of the wind's direction about two hundred were correct, seventy doubtful, and thirty wrong.

II. *The Wind's Force.*—This is always very vaguely prophesied, being indicated by such adjectives as "light," "moderate," "fresh;" or by such nouns as "airs," "breezes," "gales." In order to subject these to a uniform test I drew up the following table of equivalents, and, looking at the success achieved by the prophecies in this direction, I am inclined to think it is not far from representing the meaning of the office.

"Airs"	=	force 1 ("a light air").
"Light"	=	forces 1 and 2 ("a light breeze").
"Breezes"	=	,, 2 and 3 ("a gentle breeze").
"Moderate"	=	,, 3 and 4 ("a moderate breeze").
"Fresh"	=	force 5 ("a fresh breeze").
"Strong"	=	,, 6 ("a strong breeze").
"A gale"	=	,, 7 ("a moderate gale").

Such terms as "moderate breezes," "strong gales," "calms," &c., were referred to the forces of which they are the synonyms.

When the maximum intensity of the wind (which I estimated and checked by means of the above-mentioned reports) was coincident with the equivalent of the predicted force, as obtained by the above rather arbitrary table, I wrote B^4 on the margin of the day's record. When it varied by one force, I wrote B^3; by two, B^2; and by more than two, B^1. These letters represent the same degree of accuracy as the A's. Here are the results for the year—

	B^4	B^3	B^2	B^1
Jan.	16	4	1	3
Feb.	15	3	2	4
March	10	5	3	5
April	11	6	1	1
May	12	9	1	2
June	12	7	3	2
July	19	2	1	3
Aug.	15	7	3	2
Sept.	18	1	0	6
Oct.	16	3	1	5
Nov.	12	5	1	4
Dec.	17	2	1	5
Totals	173	54	18	42

To put it roughly, out of two hundred and eighty forecasts of the force of the wind, a hundred and seventy were correct, seventy were doubtful, and forty were incorrect—a result which is, considering the arbitrary character of the test, satisfactory in the highest degree.

In testing the predictions of the general state of the weather, rainfall temperature and humidity had all to be taken into account, and as each of these will need separate treatment I will leave their consideration for another paper.

Two of the ventilators on the District Railway, and these the two nearest to the Houses of Parliament, have been removed, and the roads restored to their original condition. One of these two ventilators was opposite to the north door of Westminster Abbey, and the other between the Westminster Palace Hotel and the Royal Aquarium.

NOTES ON BOOKS.

The Parallel Roads of Glenroy. By *Jas. Macfadzean* (J. Menzies & Co., Edinburgh). A book containing much interesting matter, and a plausible but not possible theory of the origin of the parallel roads. If Mr. Macfadzean will take the pains to calculate, or if he has not the requisite mathematical knowledge, get some one to calculate for him, how much the position of the earth's axis of rotation could possibly shift within the earth herself, with the fullest conceivable action of such causes as he suggests, he will find that such change of position can in no possible way have sufficed for such work as he wants from it.—Breeding Horses for Use. By *F. Ram* (Civil Service Printing Co., London). If we consider the wretched breed of horses now existing in the region where Virgil in his famous third Georgic described unmistakably a very fine animal we may infer that the reverse process suggested by Mr. Ram would greatly improve the various races of horses used for different purposes. He proposes practically a system by which only the best of each class should be used for breeding.—Modern Household Medicine. By *Chas. Rob. Fleury, M.D.* (2nd Edition) (E. Gould & Son, London). A most useful book, conveying just so much information to the non-professional student as he requires, in plain untechnical language, while saying nothing which could tend to encourage amateur practice. The professional medical man, indeed, so far from objecting to the presence of this work on the bookshelves of a house he visits, would in most cases be glad to know that a patient or those around had in such a work the means of recognising the meaning of certain symptoms and the necessity—where it arises—of calling in professional aid in good time. Apart from the medical information conveyed by this work, the very sensible advice and suggestions respecting habits of life, diet, use of stimulants, clothing, mental emotions, gymnastics, and so forth, will be found deserving of the most careful study.—*Practice of Medicine.* By M. Charteris, M.D. (3rd edition (J. & A. Churchill, London). A work intended specially for students; handy and practical. We see here the real value of the technical method of describing scientific matters for scientific students,—it secures brevity and clearness. Non-medical persons interested in scientific matters, and accustomed to the scientific manner of speaking, will find this work interesting and instructive.—The Diseases of Children, by *Armand Semple*, B.A., M.R.C.P., &c. A work devoted to the diseases of children, and intended chiefly for practitioners and students. But, although in parts largely technical, the work is one which intelligent parents would do well to read. It is full of useful knowledge respecting the habitudes, development, and diseases of children.—Qualitative Analysis, Notes on. By *H. J. H. Fenton*, M.A., Demonstrator of Chemistry in the University of Cambridge. (*University Press*, Cambridge). A very useful work, directing attention to the meaning of each process and the nature of each reaction. We attach great importance to the principle underlying Mr. Fenton's method. Chemical analysis is too often regarded as a series of mere mechanical operations, whose scientific meaning is overlooked.—When Did Life Begin? By *G. Hilton Scribner* (Charles Scribner's Sons, New York.) An interesting little work which will remind many readers of the ideas of the French astronomer Bailly. After carefully considering the evidence Mr. Scribner concludes, and not without good arguments in support of his view, that life began within the arctic zone, "earliest in cooling down to the heat at which life could exist on the earth," and therefore presumably, "first to become fertile, first to bear life, and first to send forth her progeny over the earth."—Navigation. By *John Merrifield*, LL.D., Head Master of the Navigation School, Plymouth (Longmans & Co., London). This is an excellent though small treatise on a subject not only important to the nautical world, but full of interest to students of astronomy. It gives a very clear account of the various methods for finding a ship's position, course, distance traversed, &c., and might be studied in this respect with great advantage by believers in a flat earth. The account is so clear and simple that any one tolerably familiar with elementary geometry, algebra, and trigonometry can follow it. We can imagine no pleasanter companion for such persons on a long sea journey than a handy book of this kind giving a clear and trustworthy account of the various methods used for their safety. We note as a point needing correction the absence of due notice of the effect of refraction in modifying the observations on which the process for taking departures by the dip of the land depends. Refraction is mentioned in such a way as to imply that the observations described only need correction for variations in the refraction, whereas they have not been corrected for refraction at all. The chapter on great circle sailing suggests the use of a gnonomic polar chart for taking great circle tracks; but although if such a chart includes the places at both ends of the journey the course is very simply indicated by connecting these places on the chart by a straight line, a gnomonic chart cannot be made to include more than a fourth of the surface of the earth without enormous increase of scale, nor one-half under any circumstances. A stereographic projection with the north pole as centre and extending to fifty degrees south latitude, would be much more useful. The course is indicated by a simple geometrical construction, thus:—Take the starting-port and its antipodes, the port to be reached and its antipodes; then, with a large bow compass or a string and pencil as described in Knowledge p. 37, sweep out (this can be done after a few moments' trial for centre) an arc through the two ports and either antipode (it will go through both when it passes through one): that is the great circle course. It is not even necessary to have the features of land and water charted; the longitude and latitude of ports and antipodes suffice to give the track, and therefore the longitude and latitude of every point along the great circle course. Prophecies and Speculations Respecting the End of the World. By *Rev. B. W. Savile* (Houlston & Sons, London). Mr. Savile was once a supporter of what he now fairly calls "the Anglo-Israel craze;" that he is not so now "goes without saying." His pamphlet "Anglo-Israelism and the Great Pyramid" even goes to the extreme length of questioning the claims of Queen Victoria to the throne of David, on which account he has been charged with "apostasy" and a number of other things (a page or so long). In the little work before us he collects a number of the prophecies in which foolish folk have believed. For fun,—to those who are amused to see what idiotic people there are in the world,—the work may be called a splendid shillingsworth.—Weights and Measures. By *John Morrison* (William Kidd, Dundee). The great trouble about weights and measures at present is that more systems are in vogue than one. Inventing a new system, however excellent in itself, is no remedy for the evil. One of the present systems *may* prevail over the rest; no new system has a chance. Otherwise Mr. Morrison's might be worth attention.—Notes on the New Principia. By *Newton Crosland*, author of "The New Principia": issued by Mr. Newton Crosland, author of said notes. Mr. Crosland "comes up smiling" after

punishment by the *Saturday Review*. He finds himself "tickled into a grin" every time he thinks of it. The grin is rather too obviously uncomfortable. In his notes on our own remarks Mr. Crosland expresses inability to understand why the centrifugal tendency varies in potency. We never said he *could* understand it. Many do not know why an engine-driver slackens speed in taking a curve, and when told it is to reduce the centrifugal tendency fail to see what connection there can be between velocity along the track and a tendency to leave the track.

THE EVOLUTION OF FLOWERS.

By Grant Allen.

II.—FIRST STEPS.

BEFORE we go any further on our upward course in tracking the development of the lily group, I should like to point out the sort of goal towards which we must be gradually tending; and I don't think this could be better done in any way than by taking three typical stages in the evolution of the lilies on the one hand, and of the buttercup family on the other. The special interest of such a comparative view depends upon the fact that while the lilies are monocotyledons, belonging to the great class whose flowers are arranged in rows of threes, the buttercups are dicotyledons, belonging to the great class whose flowers are arranged in rows of fives. Yet in spite of this primordial distinction, we shall see that the two groups progress along exactly similar lines, and that in each we can find almost exact analogues of the grade of development reached at each stage by the other. A tabular statement set side by side will enable us to observe this close parallelism in the clearest and most interesting manner.

Buttercup Group.	Lily Group.
1. The Buttercup has: five distinct greenish sepals; five coloured petals; numerous whorls of stamens; numerous whorls of carpels, all separate, and each containing one seed.	1. The Water-Plantain has: three distinct greenish sepals; three coloured petals; two whorls of stamens; numerous whorls of carpels, all separate, and each containing one seed.
2. The Columbine has: five coloured sepals; five coloured petals; numerous stamens; five carpels, forming a single group, and each containing several seeds.	2. The Tulip has: three coloured sepals: three coloured petals; two whorls of stamens; three carpels, united into a single capsule, and each containing several seeds.
3. The Monkshood has: irregular flowers, with five very peculiar coloured sepals; two to five very peculiar coloured petals; several stamens; and only three united carpels, each containing many seeds.	3. The Orchids have: irregular flowers, with three very peculiar coloured sepals; two very peculiar coloured petals; one or two stamens; and three united carpels, each containing many seeds.

This little comparative table must serve us for the time being as a rough and general guide to the direction that evolution in flowers has generally taken. The why and wherefore of each upward step must be a matter for future explanation piecemeal.

And now let us hark back to our friends the water-plantains, and consider further what are the peculiarities which each of the best-known British kinds presents after its own fashion.

We have in England two other species of Alisma besides the one which formed the text for our first paper. One of these, the lesser Alisma (*A. ranunculoides*), a much rarer plant, is so remarkably like a buttercup that Linnæus gave it its Latin name in commemoration of the close resemblance. And, indeed, the name conveys more meaning than any mere outer likeness of parts could convey: it testifies to the great botanist's underlying appreciation of the fact that the two flowers are in reality exactly analogous to one another. The lesser Alisma holds among the lily group of three-rowed or trinary flowers just the same place that the buttercup holds among the larger group of five-rowed or quinary flowers. In both, the carpels are very numerous, and they are arranged, not in a ring round the centre of the flower (as is the case with the water-plantain), but in a big globular head, which forms a central boss inside the petals and stamens. The meaning of this curious arrangement is probably this: the whorls of carpels have become so numerous and so crowded that they have crushed one another out of shape altogether. In the water-plantain, as a rule, if you count the carpels, you will find there is a rough approximation to an arrangement by whorls of three; either there are eighteen, that is to say, six whorls (the smallest number I have ever noticed), or there are twenty-one, twenty-four, twenty-seven, or thirty,

Alisma ranunculoides.

that is to say, seven, eight, nine, or ten whorls (the largest number I have ever noticed). Often enough one or two carpels have become abortive, and so the number test fails; but, even so, it is always easy to see that the carpels fall into three rough groups, each group representing the carpels of six or seven separate whorls.

But nothing of this sort can be traced in the lesser Alisma. There the carpels are simply all jumbled together into a head, as in the buttercup, and no trace at all is left of the primitive arrangement in rows of three.

Now, this singular resemblance of the lesser Alisma to the buttercup is not a mere casual accident, but is due to the fact that both plants stand at the very bottom of the evolutionary scale in their own division of flowering plants. We cannot say how or why the primitive flowering plants split up into the two great bodies (monocotyledons and dicotyledons) respectively provided in their most regular forms with whorls of three or of five members each; and we have now no links remaining between the two bodies,

though there are a few plants in each which approximate slightly in one or two points to the peculiarities of the other. But we may be pretty sure that the Alismas and the buttercups represent a very early stage of the two bodies shortly after they had begun to diverge from one another in the direction of their own particular peculiarities. Other plants in each great body have undergone wider and ever-wider changes in adaptation to altered circumstances; but the Alismas and the buttercups have continued to preserve the same features unchanged during all the long series of intervening ages. They stand to other flowers in somewhat the same relation that the Australian black-fellows stand to the remainder of humanity, or that the duck-billed ornithorhyncus stands to the fully-developed mammals.

Alisma natans.

As I intend these notes on the lilies to be a sort of explanatory companion to the ordinary botany books, it may be well to say a few words as to some other points in the English Alismas. The lesser Alisma has larger and more solitary flowers than the water-plantain; it trusts more to size and less to number than its commoner ally. Its leaves also, when submerged, often have no blade; there is no more of them developed than the bare leaf-stalk. This is a common peculiarity of submerged plants, which almost always have very long, narrow, or much-divided waving foliage. Leaves of such a sort, I believe, are better adapted for catching and utilising the carbonic acid in the water than the large broad flat blades which succeed best in the open air and sunlight.

Our third English species, the floating Alisma (*A. natans*), found only in a few western counties, displays the peculiarities of a born water-plant in a still more striking degree. It has long lithe stems which trail at great length through the still waters of quiet ponds. It has adapted itself to an aquatic existence far more thoroughly than its two half-marshy neighbours; yet it is very closely allied to the lesser Alisma, from which it only differs in habit and in one or two minute or unimportant particulars. Like a great many other thorough-going water-haunters, the floating Alisma has two kinds of leaves. The one sort, though small, is broad and oval in shape, and lolls lightly on the surface of the water, where it can catch the full sunshine unimpeded and drink in the free carbonic acid at all its pores. The other sort grows below the water, and is reduced to a mere footstalk, sometimes slightly dilated at the end, where the blade or expanded part of the leaf ought to be. Evidently, the development of the leaves here depends entirely upon the question whether they can reach up to the free air or not; the root leaves being always submerged and narrow, while the leaves that spring from the upper joints are floating and oval in outline. Exactly the same sort of thing happens again with the water-crowfoot (*Ranunculus aquatilis*) which is a water-haunting buttercup, exactly analogous to the floating Alisma; only in that case, while the upper floating leaves are broad and five-lobed, the lower waving ones are reduced to numerous long, narrow, much subdivided segments. Observe in every case that the floating leaves are analogous to the broad round foliage of the water-lily, and the submerged ones to the hair-like waving foliage of water-milfoil, and other common freshwater weeds. Almost all aquatic plants have leaves belonging to one or other of these types, or else both to one and the other in different parts. Botanical readers will remember that the pondweeds (*Potamogeton*) offer many excellent examples of all three conditions. It is interesting to observe, however, in the case of the Alismas, that the lesser Alisma preserves for us an intermediate stage in the establishment of this peculiar aquatic habit.

The carpels of the lesser Alisma have four or five little ribs each; those of the floating Alisma have from twelve to fifteen. Can anybody say why this should be so? It is one of the numerous small points of difference one always finds even between closely-allied species, points which are generally far more difficult to account for than more conspicuous matters of habit or structure, in obvious adaptation to the circumstances of the plant.

The flowers of both these kinds are nearly identical. As in most other of our English water-plants, they are whitish in colour, a peculiarity well observed also in the water-crowfoot, which (with its variety the ivy-leaved crowfoot) is our only white British buttercup. I suppose white is a favourite colour with waterside insects; certainly it is a very conspicuous one among the lush green leaves of most aquatic plants. Both in water-plantain and water-crowfoot, the petals are yellow at the bottom, which shows that yellow was their original colour; for petals always acquire new tints from outside inward. The water-plantains secrete honey at the root of the stamens, in little round drops, which can be easily seen with a small pocket lens. All the English species are fertilised by flies, especially flies of the family of Syrphidæ, the very same insects that visit and fertilise the water-crowfoot. This is a very important fact as helping to explain the close similarity in colour between these various water-side weeds. It is also interesting to note that when floods are out, the flowers of water-crowfoot and of floating Alisma both alike remain closed under water, and fertilise themselves, the closed petals compelling the stamens to shed their pollen on the stigmas of their own blossoms.

Gossip.

Among nearly a hundred more letters about our proposed change of size and price, only two have been unfavourable,—and these kindly. We note in answer to several correspondents that the size of the page will remain unchanged. At present, we only remark of the change, which will begin with the first number of March, that apart from added matter, it will enable us to keep promises which we made in the hope that they could be fulfilled without change. It is all very well to promise serial papers on such and such subjects which would just fill our present space; but when new subjects are constantly arising which though not needing serial treatment yet themselves form a continuous series, the promised articles are crowded out. A part, therefore, of our increased space will be occupied by those papers on "How to get Strong," "Light Sifting," and so forth, which we have been long anxious to resume or begin, but for which we have had insufficient space. Another portion of the increased space will be taken up in lengthening our weekly supply of matter relating to special subjects. For the rest we are promised papers on Geological, Optical, Mechanical, and other matters of general interest, for which we have long been anxious to find room. From the tone of nearly all the letters which have reached us, we deem it hardly necessary to say that from the beginning we have sailed as near to the wind as we dared (rather nearer at times than we deemed altogether safe), in the way of providing for our readers as much of what a correspondent calls "readable matter" as possible. We shall continue on this course as long as we may.

Dogberry never said anything finer than a man about whom recently we have seen a good deal (one can hardly tell why) in the papers. He was rebuking, deservedly as it seems, a certain rude man, and had expressed a very proper feeling about the fellow's bad manners. "Will you repeat that before those gentlemen there?" said the man. "Yes, I will," was the odd reply, "before *any one who does not know how to behave.*" A strangely chosen audience! He must have afforded a fund of amusement to those about him. So probably did the older Dogberry, though once he seemed tedious.

I have to cry "Peccavi." A correspondent, "A. McD.," some time since rather roundly made fun of a statement made in all good faith by Mr. J. W. Robertson. The latter, in letter 1,101, defended himself pleasantly, and received my apology for a letter which, however, was not intended, I felt sure, offensively. Mr. "A. McD." then wrote a courteous and pleasant letter apologetic both to Mr. Robertson and myself. This I *thought* I had forwarded to the printers with a little note of my own explaining that no apology to me was necessary, and mentioning how my own experience enabled me to understand that while no offence was intended, the sense of the humorous aspect of the matter had been too strong for him. Unfortunately, I had *not* it appears sent the letter. At least they knew nothing of it at the office. As it is far more likely the mistake is mine than theirs, I hereby express both to Mr. Robertson and to Mr. A. McD. my regret that my carelessness should so long have delayed the pleasant ending of this little matter.

Among the inaccuracies which Professor Ruskin attributes to scientific people is one which singularly illustrates the contempt with which he has allowed scientific teaching to pass unnoticed by him, and the scorn which he has then felt for what he has not cared to examine. The ignorant scientific person, in this particular case Prof. Tyndall, uses the words "vibration" and "undulation" as synonymous. Then Professor Ruskin defines vibrations and undulations, about as inexactly and even incorrectly as is possible. But the point to be noticed is not the incorrectness of an art-critic person, (to follow his method) but the circumstance that neither Professor Tyndall nor any one else, except Professor Ruskin, has ever even thought of the possibility of confounding "vibrations" the (proximate) cause with "undulations" the effect.

But Professor Ruskin has not contented himself with criticism of the descriptions and explanations given by scientific men; he has attempted scientific description and explanation himself. We find that he suspects the prismatic clouds to consist of comminuted water or of ice while the non-prismatic clouds consist of cloud-vapour. What is cloud-vapour? It cannot be aqueous vapour, for we know that to be as transparent as dry air. What then is it?

But he makes up for all such mistakes, by his sound precepts about troubled passions and lip-insolence Even though the precept be not illustrated in his case by practice it is well worth attention.

I find I was mistaken about my election to the St. George's Chess Club. It appears that a remark of mine ("I wish I could be a member," my words probably were), really expressing my regret that multitudinous engagements prevented me from seeking election into our leading chess club, was misunderstood to signify a wish that I might be elected. So my election was in reality an act of kindness and courtesy on the part of those who arranged it. It will be imagined how regretfully I find that I had misapprehended the matter. Yet my mistake was a very natural one. Not a word or a line had ever been addressed to me by any member of the club on the subject of election (except some three years ago by Herr Gümpel to whom I mentioned that though it would be most pleasant to me, I could not join, because I scarcely ever had an hour free for chess play) until the time when I received from the secretary the news of my election,—my name having been proposed by a gentlemen with whom I have not exchanged a dozen sentences in my life. I had an idea how the matter might have arisen; but letters received from that gentleman and from the secretary seemed to negative that idea,—really (as it now appears) correct. I can only express my warm thanks to those who procured for me the honour of election to so deservedly distinguished a body, and my regret that for want of a few words of explanation I was led—in the long run—to entirely misunderstand the matter.

My remark about the place where the club's rooms are has been misapprehended. It by no means signified that the club is little known,—the idea is indeed absurd—the club is well known on the Continent and in America as well as through the length and breadth of England. Not to know exactly where its rooms are argued only myself unknown in its purlieus, which is exactly what I meant to express.

A Spanish correspondent addresses Mr. Sala (see "Echoes of the Week" in the *Illustrated London News*) as to the meaning of the word "jerry mander" used in "el ultimo discurso de Don Josef Chamberlain a Birmingham." The Castilian has looked in all manner of English dic-

tionaries for the word without success; and Mr. Sala has at his instigation looked into others, with no better results. "Nor is it in Professor Scheele de Vere's 'Americanisms,'" he writes. I have not the latter work, and I am away from my books of reference. But I think I can explain why neither Mr. Sala nor the inquiring Spaniard have found the word in question. There is in fact no such word under the "J's" in any correct dictionary. The word is Gerrymandering, and the origin is in this wise. When political disputes were in fierce progress throughout the United States (come, I think no one will dispute that time reference) a plan was devised for so dividing up Massachusetts as to ensure a large majority of elect favourable to the side which was at the moment in power—the Democratic side. This was managed by so arranging that two or three unfavourable representatives should be elected by overwhelming majorities, so that a great number of the voters on that side were used up in securing those few elections; while on the other side representatives were to be elected by bare (but just safe majorities). Thus supposing 100,000 Democratic electors and 120,000 Republican, and the regions where these last were mostly gathered to be well known; then by suitably arranging the district, it would be possible to distribute voters for ten representatives thus:—(1), 18,000 R., 4,000 D.; (2), 19,000 R., 3,000 D.; (3), 17,500 R., 4,500 D.; (4), 9,700 R., 13,500 D.; (5), 9,300 R., 12,500 D.; (6), 9,500 R., 12,700 D.; (7), 9,100 R., 12,300 D.; (8), 9,300 R., 12,500 D.; (9), 9,400 R., 12,600 D.; (10), 9,200 R., 12,400 D. With such arrangement, or something akin to it, three Republicans only would be elected, as against seven Democrats, though the district really contained a large majority of Republican voters. With somewhat more daring in the application of the method two Republicans or even only one might be returned from the district, as against eight or nine Democrats. But naturally when a district had been specially divided up for this purpose a map indicating the boundaries of the various voting districts had a strange appearance. A map of the sort prepared on a plan which was supposed (wrongly, it is said) to have been favoured by Governor Gerry, had so strange an appearance that an artist by a few touches made it resemble a picture of a salamander. "A Gerrymander, we may call it," said the editor of a Republican paper in whose office the map hung. The name has remained in America to this day. Now that Mr. Chamberlain has used it in the old country we may expect the word to be often heard, especially as the thing exists here though not perhaps in quite so extravagant a plan as was illustrated by the original Gerrymander map.

In a treatise on the Bath Waters, by Joseph Hume Spry, Surgeon, we come across a passage from an old work called "Leland's Itinerary," (vol. 2). "Or ever I cam to the Bridge of Bath that is over Avon, I cam down by a rokky hille, fulle of fair springes of water; and on this rokky hille is sette a long streate, as a suburbe to the Cyte of Bath; and in this streate is a chapelle of St. Mary Magdalen. Ther is a great gate with a stone arche at the entre of the bridge. The bridge hath V fair stone arches. Betwixt the bridge and the south gate of Bath I markid fair medowes on eche hand, but especially on the lift hand, and they ly by south-west on the town. The Cyte of Bath is seete booth yn a fruteful and pleasant botom, the which is environed on every side with greate hilles, out of the which cum many springes of pure water, that be conveyed by dyverse way to serve the Cyte; inasmuch, that leade beyng made ther at hand, many houses yn the towne have pipes of leade to convey water from place to place.

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

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The Editor is not responsible for the opinions of correspondents.

No communications are answered by post, even though stamped and directed envelope be enclosed.

THE AFTER-GLOW.

[1119]—I am very much indebted to Mr. Howarth, for his communication (letter 1,105, p. 76) in reply to what I have previously said in these columns concerning the recent wonderful after-glows; and I may avail myself also of this opportunity of thanking Mr. Clapham and Major Hawkins-Fisher for very interesting private letters which they have addressed to me on the same subject. From Major Fisher's observation of the Christmas sunset, it must have been one of almost unparalleled splendour, and I can quite conceive, from his description, that the fiery glare of the after-glow must have penetrated the thick fog which covered our Sussex sky, and so imparted that ruddy colour to it which appeared so strange and unnatural. Mr. Clapham was observing with Mr. Kershaw (letter 1095, p. 4) at the time when this phenomena was apparent in full daylight. In his case it would seem to have assumed rather a different form to that which it presented to Mr. Howarth; inasmuch as he describes it as a kind of rosy glow or halo, with its inner edge about 5° from the sun, and its outer boundary some 20° to 25° outwards. Ice spicules would account for its appearance between 20° and 30° from the sun, but its approximation within 5° puzzles me. It is a matter of history that the sky was ablaze when Titian died. Have any of the readers of Knowledge access to records of voyages at that date? because early travellers always noted such phenomena as volcanic eruptions carefully.

William Noble.

Forest Lodge, Maresfield, Uckfield, *Feb.* 13, 1884.

SKY-GLOW (EXTRACT FROM LETTER).

[1120]—"The light in the sky is not only at sunset but at sunrise; in fact, is more brilliant in the morning. About an hour before sunrise it appears in the east. A few months ago, when staying at Patrysfontein, after it was almost dark, this light would appear, and so bright I could take my work or book and have nearly an hour's light."

Graaff-Reinet, Cape Colony, *Jan.* 1.

GRACE'S SPECTROSCOPE.

[1121]—Since my reference to Grace's spectroscope in letter No. 1052, Vol. IV., Mr. Browning has kindly given me an opportunity of trying the new form of this instrument, and I have no hesitation in saying that it is eminently adapted for the purposes of a hygro-spectroscope. The dispersion being the same as in "The Rainband Spectroscope," observers will find no impediment to the comparison of results in the two different forms.

F. W. Cory, F.R. Nut. Soc.

FEAR OF THE UNFAMILIAR.

[1122]—After I had recently closed and sent a letter containing some observations relative to Mr. Grant Allen's view of instinctive fear in certain animals with regard to others, I remembered a curious circumstance which happened to me in Delhi, in November, 1881. I had gone to the Chandni Chowk (or large bazar there) to make some purchases, accompanied by my dhurzie (or native tailor). All my commissions were executed; just as this native (a Mahomedan from the Punjaub) was assisting me into the carriage, a

native boy, about eight years of age, passed us; he was a perfect albino. I shall never forget the expression of terror depicted on my servant's face; he stood as if petrified for the moment. This was to him evidently a new sensation. He turned a ghastly colour, till I reassured him by explaining what this child was, and that there was nothing to fear. May not the effect produced on certain animals by the sight of others which are new or strange to them, as in the case of monkeys in the Zoo who have never seen a snake, be somewhat similar in its nature to this? My Mahomedan tailor was decidedly intelligent, clever above the average in his vocation, and a most faithful servant. May his shadow never be less!

Cosmopolitan.

STRANGE RESUSCITATION.

[1123]—A trawling vessel, the *Tempus Fugit*, of Lowestoft, arrived at that port on Wednesday, Jan. 9, about 7 a.m., having made the last catch of fish about 8 a.m. the previous morning. At 8 a.m. on the 9th ult. these fish were landed in Lowestoft Market, twenty-four hours having elapsed since they were caught. I picked up a fine specimen of plaice ("platessa") which was frozen quite rigid. It accidentally dropped from my hand and fell with a smart blow on the vessel's deck, when it immediately commenced jumping about quite as briskly as a fish just out of the water, and continued doing so for upwards of an hour.

This phenomenon is well known by most trawl fishermen, and is by no means an isolated case.

As I cannot understand how a fish that has been frozen for twenty-four hours, and then, by receiving a sharp blow, will exhibit—at least as far as its movements are concerned—all the appearance of a freshly-caught fish, I shall be pleased if any of the readers of Knowledge can throw any light on this subject.

John Hame.

TRICYCLING UP HILL.

[1124]—Mr. Browning seems to meet with the most extraordinary accidents while riding tricycles. I have ridden one for eighteen months (a "Salvo"), and have never met with the slightest mishap. What can Mr. Browning mean when he says, "The machine should not have been turned or allowed to turn when riding up a stiff hill"? Why, any tricycle-rider will know that the easiest way to ride up a steep hill is to keep zigzagging or turning about from one side of the road to the other, in the same manner that one will see the driver of a horse that has a heavy load to pull up a steep hill—in fact, the horse will adopt the plan of his own accord! What sort of a machine must it be that will not work in this way going up hill?

Subscriber.

EARLY EMOTIONS.

[1125]—I have observed a blind kitten, not three days old, spit, after the manner of its kind, at a dog to whom it was shown. Such an act seems to involve sensations and emotions of considerable complexity, and I must leave the analysis of them to others; but I may mention that there were no signs of anger or excitement on the part of the dog, who was rather frightened than otherwise. The object of the introduction was to explain to him that the kitten was a recognised member of the household, and was not to be molested.

T. S. L.

P.S.—On no other occasion have I ever seen so young a kitten act thus.

LETTERS RECEIVED.

H. B. Lindsay. The closing poem to my lecture on the "Life of Worlds" is Richter's dream, as translated by De Quincey. It closes my "Universe of Stars." Goethe's lines, by which I usually close my lecture on the Moon, beginning "See all things with each other blending," will be found in Anster's translation, publisher's name I have forgotten. I do not know the lines beginning "Roll on thou sun, for ever roll!" and should be much obliged if you could send them: albeit the first line reminds me rather grotesquely of Gilbert's address to the earth, beginning "Roll on thou inky ball, roll on! what though," &c.; and ending, "Never *you* mind, roll on! (*It rolls on.*)"—Senex. Thanks: I agree with you muchly. In passing, let me express my sympathy with your remark (proverbial quite), "a thousand mosquitoes cannot kill a man; but, my word, they can worry him, *some*." You would still better know how applicable it has been if you could see arrayed the multitudinous suggestions made me in the past, and even lately. Why so much about the stars, cooking, flowers, insects, the microscope, the telescope, happiness, Chess, Whist, and now chiefly the tricycle. Then if one of these or other subjects gets less than usual, why are you dropping out flowers, insects, the telescope, happiness, &c. The writers themselves on these various subjects have been quite good and patient with me, when I have had to limit their space,—with one recent and rather "loud" exception. But many are ready to raise an uproar for them.—R. Dalziel. Thanks. That is the way to view matters. On the last point; I also think a man is about the best judge of what he can do; also of what he cannot. Yet he may have to learn from others what his work is worth. It is not by the words but by the actions of others that a man is taught what his capacities and shortcomings may be. I tried a certain line of writing eighteen years ago, which I thought I could manage fairly well: no one *told* me I could not, neither did I ask any one to tell me; but I was shown in a very practical way that what I was trying was not in my line.—T. Canaway. Yet another "Yes." Thanks. We certainly do not mean to alter the page.—Harry Wilson. And another. The evidence and reasoning about matters rather voluminous. Chiefly thus. In a given part of the sky, carefully watched, a certain number seen. For decrease of size a certain increase in number. But some telescopic. Hence inference as to actual number for whole sky, and thence for whole earth.—C. J. Caswell. Another in favour of the change. I do not know how my old friend F.R.A.S. came to overlook the occultation of Venus.—X. Another. As to Index, quite agree with you. It will come out in future with the second or third number following completion of volume.—H. Poole. Thanks. Your letter marked for insertion.—R. B. Cook. Simply crowded out. With the new arrangement there will be no such trouble. The absence of initials arose from the intention to insert.—G. A. Brown. Thanks. Every correspondent thus far answered here has approved the change of size,—except one who sent a letter for insertion, and had no occasion to refer to the matter.—H. Romeike. Many thanks. That pleasant and kindly notice had not reached me. If it had been unpleasant I think it would. But I am inclined to modify the opinion I had formed. I believe many would be benefited by an arrangement bringing such notices to their attention.—W. J. Trouton. The results are almost identical if almost be taken with a tolerably wide meaning. "We can't quite almost generally tell," as an American hymn remarks. But your relations will not do. My old acquaintances (the real distances) are far better. It is worthy of notice, however, how singularly the planetary relations you deal with and others can be fitted together with a little ingenuity. See Prof. P. Smyth on the Pyramid for other instances.

Our Parador Column.

LIGHTNING.

A MOST remarkable attack upon the universally received theory of lightning discharges has been made by Colonel the Hon. Arthur Parnell, late R.E., who on the 21st ult. read a paper on "The Action of Lightning Strokes in Regard to the Metals and Chimneys of Buildings," before the members of the Royal Institute of British Architects.

The paper was the result of four years' research into the nature and action of what he termed "thunderbolts," instead of lightning. He had 506 cases selected out of 1,145 records of lightning strokes, ranging from Jan. 24, 1665, to Nov. 23, 1883. In all these cases metals were present in the buildings struck, or chimneys were in the immediate vicinity. The cases quoted were, however, wanting in point, as even when a lightning conductor or protector was present, he failed to give any particulars as to its condition. In deducing his practical conclusions, he stated that he desired especially to guard against advising the members of the Institute taking *any measures at all* to safeguard buildings from thunderbolts.

He laid it down as a general principle that *the use of metal in any form* inside and outside buildings should be minimised. He condemned as wholly useless all metallic external appliances such as—

I. Lightning conductors.

II. Vanes, weathercocks, finials, crosses, balls, and spindles.

III. Bells and clocks in towers, and in elevated parts of buildings.

IV. Iron ridge castings.

V. Metallic balconies (as also metal chimney-pots, flashings, hips, and other lead work on roofs; eaves-gutters, rain-water pipes, wire-guards to large windows, iron window-bars, &c.).

Inside of buildings he condemned as dangerous the employment of metal, and

VI. Large pier-glasses.

VII. Gildings (together with organs, pianos, safes, iron bedsteads, gas-pipes, and water-pipes).

For substitutes he would use earthenware, terra-cotta, Portland cement, glass, asphalte, &c.

He considered gas-holders, oil-tanks, railway stations, and temporary erections of corrugated iron, as special cases, which he would meet by connecting the iron framework to the ground at one or more places, and provide the most elevated points of such metalwork with short spikes, very sharply pointed, so as to convert the whole mass into what he calls "an *electric tap*, ready to eject the whole charge that may accumulate in the ground near its base, in virtue of the physical property of metal points to throw off and scatter electricity."

Chimneys should be built without metal, but since they were a necessity he suggested:—1. That the metal work on the *lowest floor* should be connected to the soil below by means of two iron bands, one at each side, securely fastened to the foot of the grate's front, and passing down through the hearthstone below into about a foot of soil. 2. To fix securely and closely to the top bar of the grate two iron spikes, three inches long, at an angle of 45°. The points to be steeled and always kept sharp.

In concluding his remarks the gallant Colonel contradicted his previous advice by urging architects to make a study of the physics of lightning strokes as a part of their professional business.

Enough knowledge of lightning strokes to enable an architect to safeguard a building could be obtained from nature alone:—"*It did not belong to the electrical engineer, whose province was artificial electricity.*" The defence of life and property from lightning seemed to rest on the labours of three classes of scientific men:—1. The meteorologists, who observed the phenomena of thunderstorms. 2. The physicists, who reasoned on the facts, and eventually deduced therefrom theories and laws. 3. Architects practically to apply the reasonings of the physicists.

As may be imagined, the reading of such a paper called forth frequent roars of laughter, which was slightly reflected in the comments of the various speakers who attempted to criticise the idea set forth. Thus Prof. Symons, secretary of the recent Lightning-rod Conference, said his breath had been fairly taken away by the statements he had listened to. He could fancy he had been carried backward just a century. Prof. Hughes confessed he had learnt a great deal that night—to wit, that lightning and electricity were different in their action; whilst another interesting point was that there were no lightning strokes now, but there were a number of "thunderbolts." He desired very much to see one and get hold of it. To talk of buildings being destroyed by lightning because they were provided with lightning-conductors was like talking of ships being wrecked by their rudders, or of steam boilers being burst by their safety-valves.

To us, the chief surprise is that a society like that of the British Architects could have afforded to devote an evening to the ventilation of such nonsense as is contained in the paper. We presume the great majority of our readers are better acquainted with the subject, and can enter into the feeling of amusement infused amongst those present who could appreciate the fun—otherwise we should have long since devoted an article to the subject.

We hear that Messrs. Wyman & Sons, of Great Queen-street, London, have a little work in the press, entitled "John Bull's Neighbour in her True Light: being an Answer to some recent French Criticisms." By "A Brutal Saxon." Rumour says that the book is likely to be as eagerly read as "John Bull and his Island," as the author, who has lived for many years in France, draws some startling pictures of French life; and his authoritative descriptions of "Horrible Paris" can hardly fail to cause a sensation on this side of the Channel.

LONG-DISTANCE TELEPHONY.—Some very successful experiments in communicating over long distances by telephone have been made by the officials of the electric telegraph department at Port Elizabeth. The first experiment, says *The Colonies and India*, was made between Port Elizabeth and Graham's Town, when the sound was freely transmitted along the line—a lively conversation being carried on. Songs sung were also distinctly heard at the other end. The Fort Beaufort line was then connected, and an extension made *vid* Humansdorp, giving a total length of communication of over 400 miles, through which the sound travelled with astonishing clearness. It is probable that an attempt will shortly be made to communicate with Cape Town by the telephone.

Our Whist Column.

BY "FIVE OF CLUBS."

THE following game was played recently at the Editor's table: it shows how a lost game may be sometimes saved if the holders of winning hands are thrown off their guard by the seeming certainty of their game.

THE HANDS.

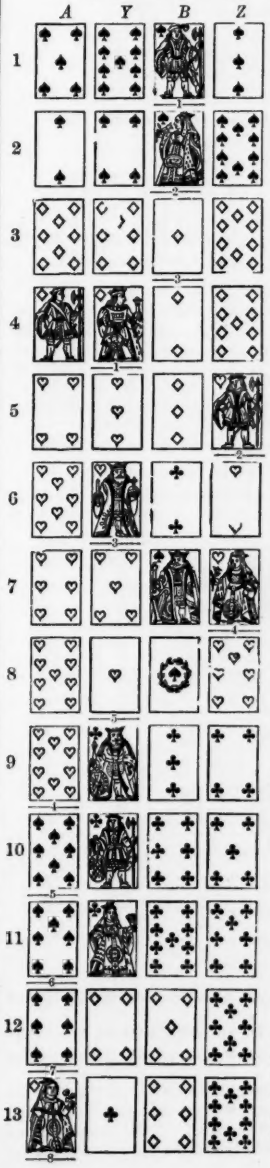
B: *Hearts*—None. Clubs—9, 6, 3, 2. Spades—A, K, Q, Kn. Diamonds—A, 6, 5, 3, 2.

Y: *Hearts*—A, K, 5, 3. Clubs—A, K, Q, Kn. Diamonds—K, 7, 4. Spades—9, 4.

A B. 3. **B** Dealer. **Y** *Y Z*, 1. **Z** *Trump. H*. 7. Leader. **A**

Z: 2, 7, Kn, Q—*Hearts*. 4, 5, 7, 8, 19—Clubs. 3, 10—Spades. 9, 10—Diamonds.

A: *Hearts*—10, 9, 8, 6, 4. Spades—8, 7, 6, 5, 2. Clubs—None. Diamonds—Q, Kn, 8.



NOTES AND INFERENCES.

1. *A* leads the penultimate of his long weak suit rather than trumps. The lead is unsound. *Y* begins a signal.

2. *B* leads the lowest of the head sequence to show *A* he holds the others. *Y* completes the signal. Neither *Y* nor *Z* has any more Spades.

3. *B* cannot go on with Spades since that would be leaving *Z* to ruff and *Y* to discard. He therefore opens his long suit. *Z* begins "the echo of the signal."

4. *Z* completes the echo. The Queen lies either with *A* or *Z*.

5. *B* discards from his long suit, strength in trumps being with the enemy.

6. All the honours are with *Y-Z*, who therefore win unless *A-B* can make two by cards. As *Y-Z* are sure of four tricks in trumps and have already won a trick in a plain suit, the case looks almost hopeless, since one more trick will win the game for *Y-Z*. *B* sees that the sole chance is that *A* may hold the long trump and be able with it to bring in his long spades. He does not begin *at once* however to get out of the way. If he had allowed the seventh trick to pass before beginning to discard his big Spades the game would have been past saving. But he had a reason for waiting to the seventh trick.

7. *Y* should at once have led Clubs. There is only one chance for *A-B*, viz., that *A* holding the winning Diamond should remain with the long trump. If he does, then as he has the three long Spades, he can have no Clubs, and will bring in his long suit by ruffing Clubs after the other trumps are out. But if he is forced at trick 7 (and he cannot refuse the force) he is powerless to save the game.

8. *Y* having blundered, *Z* follows suit. They thought the game so sure that no care was wanted. "A mere 'walk over,'" said *Z*, as he led; and so it was, only the walking was done by the other side. The discard of the Spade King at trick 7 should have shown *Y-Z* their danger. That was why *B* delayed the significant discards.

9, 10, 11, 12, 13. *A B* walk over.

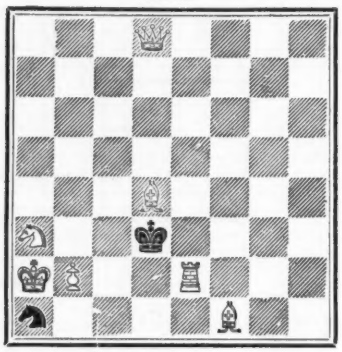
Our Chess Column.

By Mephisto.

SELECTED PROBLEM.

By G. J. Slater.

Black.



White.

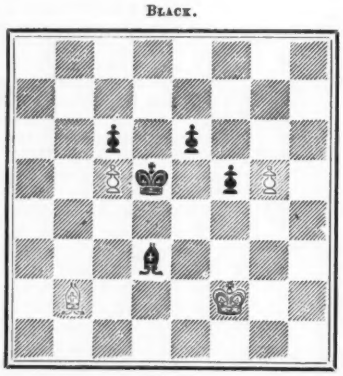
White to play and self-mate in seven moves.

1. P to Kt4	1. Kt moves
2. B to R sq (ch)	2. Kt to Q5
3. Kt to Kt sq	3. K to B5
4. R to QKt2 (ch)	4. Kt to K7
5. Q to Q6	5. K to Kt4
6. Q to K6	6. K to R5
7. Kt checks	7. Kt takes Kt, mate

The following position and ending, occurring in a match game played on Saturday evening, the 9th inst., between Mr. Hoon (playing for the North London Club), and Mr. H. Selfe Leonard (playing for Greenwich), are illustrative alike of the mischief of "trying to win a drawn game," of the importance at times of reckoning with minutiæ of arrangement, seemingly insignificant, and, still more, of the occasional rashness of pawn capture :—

Mr. Hoon.

Black.



White.

Mr. Leonard.

K to K3	B to QKt8 (*a*)
K to KB4	K takes P (*b*)
B to K5, and wins (*c*)	

NOTES.

(*a*) Not very obvious, but this choice of sq for the B proves curiously unhappy.

(*b*) The sacrifice here by Black of his King's Pawn, instead of his greedy capture of the White Pawn would still have secured the draw, but he missed his danger.

(*c*) For, curiously enough, the sole remaining White Pawn is too much for the three remaining Black ones, as was the last of the Horatii in Roman story more than the equal of the three maimed Curatii. Though in the middle of the board, he can be caught by nothing before Queening, so effectively is Black's Bishop *hors de combat*, thanks to his own Pawns. It would have been otherwise had he gone, for instance, to B2, whence in three moves he could have commanded KKt sq.

SOLUTION.

Problem No. 114, by F. J. D., p. 92.

1. B to Kt8	K to K3	Or,	K to B4
2. K to K3	K to B4	Kt takes P	Any
3. Kt to Q4, mate		Kt mates	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

Parson.—1. *Chess Monthly* and *British Chess Magazine*. 2. They are mostly open to everybody. Write to the Hon. Sec. of the particular club you wish to join. The St. George's is the foremost.

Ch. E. Bell.—If 1. Q takes Kt, B to QB3. 2. Q to K6, B takes B. 3. Q to QB4 (ch), K to K4, and there is no mate.

Henry Bristow.—Solution correct.

MR. R. A. PROCTOR'S COURSE OF LECTURES.

1. LIFE OF WORLDS.	4. THE PLANETS.
2. THE SUN.	5. COMETS.
3. THE MOON.	6. THE STAR DEPTHS.

See Advt. Pages for full Syllabus.

The following arrangements are complete: the numbers in brackets referring to above list.

BRISTOL (Colston Hall), Feb. 22, 26, 29; March 4, 7 (the full course).
GLOUCESTER (Corn Exchange), Feb. 27, 28 (1, 2, 3, 4).
TAUNTON (Victoria Rooms), March 5, 6 (1, 2).
SURBITON, Feb. 25, March 3.
BIRKENHEAD, March 10.
ALTRINCHAM, March 11 (5).
CHESTER, March 12, 13 (1, 2).
HAVERSTOCK-HILL, March 14 (2).
BLACKHEATH, March 17, 18.
REIGATE, March 19 (2).
HITCHIN, March 20 (5).
UXBRIDGE, March 21 (1).
LONDON (Brixton Hall) March 28, April 1, 4 (1, 2, 3).
" (Memorial Hall), March 24, 27, 31, April 3 (1, 2, 3, 4).
OXFORD, May 12, 13, 19, 20.

Note.—*All communications respecting Lectures should be addressed to Mr. John Stuart, Royal Concert Hall, St. Leonards.*

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